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Discussions

Interactive comment on "The importance of better mapping of stream networks using high resolution digital elevation models – upscaling from watershed scale to regional and national scales" *by* Anneli M. Ågren and William Lidberg

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This paper is probably one of the worst I have ever been asked to review. The only interesting information I got by reading it was to discover the NILS dataset, which was not collected by the authors. My recommendation is to reject this paper.

1. Formal problems:

- Poor English, with a lot of typos, grammatical errors, improper expressions in written English ("it's"), and some very awkward sentences, so awkward that the reader loses the sense.

- The authors use terms and abbreviations that are not canonical in environment science, without proper explanation, e.g., "breaching", Matthews Correlation Coefficient, Residuals (used in a very odd way in this paper), "local topography was calculated as a standard deviation of the digital elevation model in a moving window" (p6, L27), IBM SPSS Statistics 24, OPLS-DA, SIMCA 14, R2Y(cum), R2X(cum), Q2(cum), loading scatter plot.

- Even "stream burning" would deserve a short explanation although classically used for a long time in DEM processing.

- The methods are very hard to follow, because of the above deficiencies, and also because the authors have no sense of structure. In particular Fig2 should not be called in the Results section but in Material and method, to illustrate the NILS dataset. This dataset, which seems to be extremely rich and interesting would deserve much more explanations. The beginning of Section 3 (p7, L4-9) gives results on the field investigation (NILS) and should go in the dedicated section (2.2). The Swedish property map should be introduced independently from the modelled stream networks as it serves as an (independent?) reference.

- The Results and Discussion sections also suffer structural problems, with some results in the Discussion (in particular the first paragraph with %, the links of which with Table 1 is not straightforward), some elements of the Discussion which repeat the Introduction, and some other elements which open future perspectives and would rather fit in the Conclusion.

2. Scientific problems:

- No clear scientific questions. The ones I gathered are at the end of the Introduction (p3, L11-12, with typo): "A research question that remains is therfore; How does one select the correct stream initiation threshold and validate DEM derived stream networks when scaling up from a catchment scale to regional or national scale? [. . .] Therefore the aim of this study is to determine the optimum threshold for stream initiation on a national scale, using the Swedish landscape at a test bench." But both questions are ill-posed since the paper is not about upscaling (the study only addresses headwater catchments in 25-km^2 squares) and looking for "the" optimal threshold for the large scale is extremely naive given recent results showing that the initiation threshold is spatially variable, and depends on several factors, like geology, land use, or climate (e.g. Colombo et al., 2007; Luo et al., 2016; Schneider et al. 2017). The authors seem to be aware of the problem, cf. the last line of the paper ("When applying the same methodology to other biomes it's necessary to adapt the models and find the optimum flow initiation threshold for that unique landscape"), so what's the point of the paper?

- Only two pieces of result! They are limited to one table and one figure with a totally insufficient caption (Fig. 3), so the reader does not even know what he/she looks at. This is far from being enough, and all the more as these elements are not properly commented.

- Circular use of the property map, which serves as a benchmark against which the modelled stream networks compare favorably, but on the other hand, the corresponding stream lines were "burnt" in the DEM used to model the steam networks with several initiation thresholds.

- Many unsupported conclusions, especially in the Discussion (subjective or speculative opinions should appear as such), and some abusive conclusions. In particular, the accuracy values in Table 1 are all very close to each other and should thus be used with caution. In contrast, the Matthews Correlation Coefficients are never very good (always lower than 0.5 which is not outstanding for a correlation coefficient) so it is misleading to conclude that "a 2 ha flow initiation threshold yielded the optimum stream network.

- The discussion misses important points, notably the influence on the results of (i) the LiDAR DEM quality, (ii) the imprecision of the stream location, (iii) the nature of the field survey which does not investigate the full squares but only their borders, (iv) the

particular geology and geomorphology of Sweden and consequences for generalizing the results.

- The title is not explicit, since it focuses on upscaling, which is absolutely not the key point of the study.

3. Cited references:

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